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SURGICAL LECTURES,

DELIVERED BY

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Theatre, St. Bartholomew's Hospital.

LECTURE 22.

The consequences of Gonorrhœa.

GENTLEMEN,

THE discharge of gonorrhœa will sometimes leave the internal surface of the urethra and attack the parts about the corona glandis, and it is then called the gonorrhœa of the prepuce. It is a circumstance worthy of notice, since it serves to show the sympathy of continuous surfaces, and prepares the mind to believe, which is indeed forced upon it by the occurrence of cases, that sores will break out upon the prepuce without their being contaminating sores—sores which break out, at a considerable period after any impure connexion, from the irritation of the prepuce which the gonorrhœa may occasion. It is not at all unfrequent to see such sores, but they ought not to be treated as syphilitic, because they are not curable by mercury; but the treatment of them I shall afterwards have to refer to.

Swelled Testicle.

If I were to mention the concomitants, or consequences of gonorrhœa, in the order of their frequency, I should say, that a swelled testicle was most frequent. It rarely occurs, however, while the clap is in full force, but when the disease is going off; and, they say, that it is owing to the stopping of the running of the clap, and that the running being again established the disease gets well. There are many urgent reasons against stopping the discharge of a clap, and this should be one of them; indeed, DESSAULT, who lived almost all his lifetime at the Hôtel Dieu, at Paris, and perhaps saw more of this disease than any other man living, (for he must have seen thousands of such cases there,) has an expression to this effect, "that it is sinful to stop the progress of the clap." This is a strong expression, certainly, but there is great truth in it. The irritation towards the orifice of the urethra sometimes suddenly subsides, and attacks the prostatic portion. A swelled testicle has often been called a sympathetic inflammation, but it is no such thing, it is not sympathetic, it is the result of a propagation of the inflammation by the *vas deferens* to the testicle. The inflammation which is set up in the prostatic portion of the urethra, communicates a like action to the orifices of the seminal ducts, which terminate there, and that inflammation is extended to the testicles. It seems to be curious, that this irritable state of the testicle endures a certain length of time and then subsides; it continues just as long as the inflammatory stage of the gonorrhœa, namely, about three weeks. There is generally some swelling of the testicle produced, but the degree of it very much depends upon the management of the case. It is no use to fight against it, or go about with such a complaint; he must lay himself up for a time and take means to reduce the irritable state of the testicle; and although the testicle may become a little harder and the scrotum inflamed, yet if the patient keep himself in the horizontal position and soothe the testicle, I have known such cases to go off with very little swelling. The inflammation of the testicle may abate, yet if the patient go about before it is quite gone,

it is very liable to return again, and be accompanied with shivering and great constitutional disturbance. It is of no use, I say, to fight against the disease, and cases are liable to relapse within a certain period of time, that is to say, about three weeks. The discharge then again returns from the fore part of the passage. Sometimes it happens that both testicles become affected; sometimes the inflammation appears to shift alternately from one to another.

Treatment of swelled Testicle.

There is irritation seated in the back part of the urethra, and there is nothing better for the purpose of relieving it than tepid bathing of the perinæum and genitals, as by flannels wrung out of warm water, or the local tepid bath, and I consider there is nothing better than the bread and water poultice. The weight of the part should be taken off, by affording it a comfortable degree of support, and a proper bag truss is a most excellent thing; but I can rarely see a good suspension bandage now-a-days, they are for introducing new fashions upon all occasions, and they have introduced new shaped trusses for this purpose. The bag of the truss ought to be attached to a circular belt by two straps, with buckles, so that they might be raised to a comfortable height, and no farther. There is generally some little febrile state induced, and then you give some James's powder, and take care to keep the bowels regular; let the patient remain in an horizontal position, and he will soon get well. It is of great importance to keep the horizontal position when any of the lower part of the body is inflamed, for example, the leg; you must have noticed the relief almost instantly afforded by laying the patient on his back. As to leeches, I say nothing of them; I do not think that they do much good; the disease appears to continue for a determinate time, and then goes away if you attend to the circumstances I have just named. This is all that I am inclined to do; and I say, that although people get better, they should not go about under a certain time, for if they do the disease will return.

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Irritation of the Glands in the Groin.

There is no person who has a gonorrhœa, that has not more or less enlargement of some of the glands in the groin; therefore, in point of precedence, I ought perhaps to have named this first. It appears that the absorbent vessels are irritated by the inflammation which is going on in the mucous membrane of the urethra, and the glands to which these vessels lead become inflamed. When you soothe the inflammation in the urethra, the glands generally subside very rapidly, provided the patient only observes rest and takes measures to soothe the parts. They do not require to be treated as venereal buboes, but the treatment employed to cure the gonorrhœa will generally remove them. You may apply tepid water to the part, linen dipped in tepid water and allowed then to evaporate, and I repeat, that the means adopted for the cure of the one will remove the other.

Irritable Bladder.

As another concomitant, or consequence of a gonorrhœa, I may mention an irritable bladder. The patient makes water very frequently, and has an unconquerable inclination to do so; the patient has, perhaps, to make water three or four times in an hour, and only a few drops may escape, when the irritation is violent, yet he feels as if his bladder were quite full; there is, at the same time, a great irritation felt at the extremity of the urethra, and the patient often pinches the part as if there was something there to squeeze out. When I was young, patients used to take castor oil and mucilaginous drinks, and things of that kind, and without much benefit; they used to remain a considerable time suffering from the disease; but had I known then as much as I know about it now, I am sure such cases would have been cut short by bathing the perinæum with tepid water and applying leeches there; by keeping the bowels regularly open, by mild aperients, and regulating the diet. This is the treatment that should be adopted in such cases, and the complaint subsides in a short time. There are sometimes knobs which form round the urethra, about the

size of horse beans, and sometimes they suppurate; they form in those parts of the urethra in which there is the most mucous secretion—as behind the frenum glandis, just in front of the bulb of the urethra, and in the perinæum behind the bulb, just where Cowper's glands are situated. I have seen such swellings increase as large as a pigeon's egg and gradually subside again. Sometimes they burst into the urethra only, or burst outwards only, and at other times they burst both inward and outward, establishing a communication with the external parts and the urethra, forming what has been called a *fistula in perinæo*. When I was a young man, if the surgeons felt any fixed hardness in these parts of the urethra, they used to prescribe mercury; but there is no necessity for this, indeed it is wrong. It is not often that the communication is established so as to form a fistula, even although the swelling should be laid open. All I have to tell you is, that if you poultice them and treat them kindly they will do very well.

Chordee.

The inflammation from the membranous lining of the urethra is sometimes propagated to the corpora spongiosa, agglutinating their cells together and producing a kind of *chordee*. This occurs sometimes in the early stages of a gonorrhœa, and produces great pain. It is the distension of the irritable urethra by a powerful and violent erection of the penis. It occurs often at night when the patient is warm in bed, and is best relieved by tepid applications to the part and an opiate at bed-time. But this chordee, of which I am at present speaking, is rather the result of the acute stage of the gonorrhœa; there is a thickening and agglutination of some of the cells of the corpora cavernosa, so that it is imperfectly distended and the erection is imperfect, the penis bent downwards and attended with great pain. This state is best removed by applying a tepid bread and water poultice to the parts, by avoiding all causes of irritation, by keeping the bowels regular, and by attention to the diet, so that the digestive organs may be set to rights. A man should not take horse exercise, as that would increase the irritation of the parts. Some small doses of aperients, as a few grains of calomel or blue pill, with a little rhubarb and jalap, with a little castor oil, are the best aperients; the bowels must be kept open, but by no means irritate the intestines by teasing them with coarse cathartics.

Strictures.

Gonorrhœas are very liable, if they be improperly treated, to lay the foundation for strictures. I have spoken of the inflammation shifting from the urethra to the external part, producing a discharge from beneath the glans; I have said that it sometimes leaves the fore part of the urethra and attacks the back part of it, or that portion inclosed by the prostate gland, and therefore called the prostatic portion of the urethra. It may leave that part and again attack the extremity; but the inflammation sometimes establishes itself there and causes a thickening of the parietes, and consequently a diminution of the calibre of the canal. Indeed the whole of the urethra is so contractile and irritable, that strictures are likely to occur in any part of it, as in all other mucous tubes surrounded by a muscular structure, as in the œsophagus, the biliary ducts, or the intestines, especially the rectum. So irritable and powerful is the contraction of the urethra, that, egad! persons can hardly hold their water a minute who have an irritable urethra, or when stricture begins to form. You have a proof of the resisting or contractile power of the urethra when irritated, if you attempt to pass a bougie under these circumstances. You must have seen this happen, that if you attempt to introduce a bougie when the urethra is irritable, that it will be grasped firmly, held so tight, that it cannot be pushed on to the bladder, but be twisted and indented in various places. It very often happens that there is a sort of spasmodic action of the urethra excited, and a temporary retention of urine is the consequence. If this should happen some little time after a man has had a clap, he goes in a great fright to a surgeon, and he proceeds to pass a bougie, but he finds that he

can get but a little way into the urethra, and when he takes it out he sees that it is very much contorted, and has several depressions on its surface. He informs the patient that he has a stricture, and a very bad one too; intimates to him that he must have neglected himself very much, and so on. The irritation subsides spontaneously, and he makes water as well as before. There could not have been a stricture, because the patient passed the urine very well a short time before, and a stricture could not have been formed so suddenly as all that. Sir EVERARD HOME has observed, that such a state of the urethra may arise from the application of a blister, and he has given a case where a bougie was attempted to be passed, and the result was similar to that which I have described to you. It has often happened, that patients have gone to some surgeon in the country and he has done what I tell you. The patient is convinced that he must have a stricture, and a bougie is passed, perhaps every day, until the urethra is made excessively irritable indeed.—They sometimes come to London for further advice, and how they stumble upon me I cannot tell; certain it is that they do so. A man comes to me and says, with a very long face, "I am very bad, sir, very bad indeed." "What is the matter?" "Oh, sir, I have got such a stricture as I fear I shall never get rid of it; I can only make water in such a small stream, and whenever I feel the least inclination to evacuate the bladder, I must do so directly," and so on. Well then, I hear the history of the case, and I find out that the bougie has been passed much too often, and that the evil is owing to an irritable state of the urethra. I do not, under these circumstances, set about passing a bougie, but I tell them to bathe the perinæum with tepid water, to take some medicine to open their bowels, to keep them regular, to attend to their diet, to take no spirits, and so on, and to keep themselves quiet and call upon me again at the end of a week. The truth is, that many such persons never call on me again; they find that they are so much better, that they can make water so well, that they put off the evil day, as they think; they have suffered so much from having a bougie passed that they are afraid of it. Well, those who do return come saying that they are very much better, but they should like to have a bougie passed. Well, I say, to satisfy you I will pass a bougie; now what size would you like me to pass? and I show them a bundle and let them take their choice. They will be sure to choose one of the smallest, and say, "Mr. ——— could only pass one of that size, sir." If I take up one twice the size and ask them if I shall try that—"Oh, Lord, sir, you frighten me, you could not, I am sure." "We will try, if you please;" and after dipping the end of it in a little oil, I introduce it very gently; it goes into the bladder without any difficulty, and they are perfectly astonished.

If this irritation become established in any one part of the urethra, it lays the foundation for a stricture. The irritability being established, a kind of inflammatory action attends it, and there is a thickening of the membrane of the urethra, causing a diminution of the calibre of the canal and producing a *permanent stricture*. As Sir EVERARD HOME has told you, you may take a soft wax bougie, and passing it on to the stricture you may, by continuing the pressure a little, take exactly the model of the stricture. The contraction is more upon the lower than the upper part of the urethra; the strictures are, indeed, half moons rather than circles; you should, therefore, when you are passing a bougie, always conduct the point of it along the upper part of the urethra; it is a point of some importance to be attended to, especially when you reach that part of the urethra which passes between the arch of the pubis. I know of no book that contains such good directions for passing a bougie as that written by Mr. WHATELY. Every young man ought to accustom himself to the passing of bougies, for I can assure you that there is great reputation either got or lost according to the degree of skill which a man possesses in this respect. Mr. W. has told you, that you should nip up the end of the bougie a little between your finger and thumb, and having thus rounded its extremity, take it as you would a

pen in your hand, and carry it on gently, keeping the extremity of it against the upper surface of the urethra; when you reach that part of the urethra which passes beneath the pubis, depress the handle of the bougie, by which you elevate its point, and it will find its way into the bladder without much difficulty.

I can never impress upon your memories a thing half so well in any other way as by the recital of cases: A surgeon called upon me from the country, to request me to accompany him to a patient who had very bad strictures, and for whom he could not pass a bougie; yet he said that he could make water in a stream, so that the stricture could not have been very bad you know; however, he became very much alarmed because he could not pass a bougie, and wished me to go with him. I learned from him what size bougie had been last past, and I took up one about the same size, and without letting him see what I did, I just nipped up the end of it a little, and by attending to the rule which I have told you, keeping the point against the upper surface of the urethra, I passed it into the bladder without any difficulty; I could carry the handle of the bougie close down upon the pubis, and that was a good proof that the other end had gone into the bladder. The patient and surgeon were quite surprised, and I believe the patient thought me a very clever fellow, when, in truth, there was not much credit due to me for passing it. Well, I did not wish to make any secret of the mode of passing it, and so I told the surgeon how it was to be done. I remember another case, of a very queer little man from the country, who had strictures for some considerable time, and he came to me complaining of the very distressing state in which he was left by the strictures; that he could only pass such a very small bougie, and that there was great difficulty even in passing that. I understood from him, that his surgeon had been passing a bougie every second day, and that his urethra was in a very irritable state, so I told him that I should not pass a bougie then, but that he should bathe his perinæum with tepid water, apply some leeches to the part, and come to me again at the end of a week. He came, and I passed a bougie nearly twice the size which had been before passed, without any difficulty. The man could hardly believe that it was done, and the expression he made use of was,—"Bless my soul, sir, I am sure you must have had a pick-lock key at the end of it." (A laugh.)

Now the opening of a stricture may lie to one side of the urethra or the other. In passing a bougie you have the point sometimes turned upwards, or to one side or the other; and what is the inference but that the stricture lies in the opposite direction to that from which the bougie is directed. From the misapplication of remedies to cure a gonorrhœa, or from the too frequent introduction of bougies to cure either imaginary or real strictures, an irritable, lurking disease is established, which gets worse by degrees; the urethra becomes very much contracted, until the stricture is very bad indeed.

Treatment of Strictures.

The treatment of strictures may be divided into two parts, Medical and Surgical; and I know that the medical treatment is of great importance. The patient should use the tepid hip bath, apply leeches to the perinæum, sponge it often with tepid water; he should also use some mild aperient medicine, not such as would irritate the lower bowels. Set his stomach and bowels right by a proper regulation of diet; for while there is any thing wrong about the stomach and bowels, the urethra will never get right. You next proceed to examine the urethra, and for this purpose you should use a soft bougie of a small size, and having done what I before recommended, introduce it very gently, and carry it on till you come to the stricture; keep the point of the bougie directed toward the upper part of the urethra, and if you find you cannot pass it on in that direction, turn it a little to one side, and then to another, and you may generally get it on. But if you take a large bougie, and pass it roughly, you will lay the patient up with a retention of urine. Pass it in

the most careful way that you may, you will be sure to excite some irritation, and never attempt to pass another until you have again soothed the urethra. Indeed I make it a rule never to do so; I never will pass a bougie for a patient until I have soothed the urethra by tepid bathing and so on. I have lived long enough in town to know the result of my own practice in this respect, and to contrast it with that of others, and I know that this is the best practice. The reason why strictures get so bad as they sometimes do is the following: A man applies to a surgeon, who has an irritable urethra or a slight stricture, he passes a bougie roughly, and brings on retention of urine. Means are taken to soothe the parts and allay the irritation, he gets better for a time, can make water better than he did before. But the complaint returns, and the patient has such a horror of a bougie, that he keeps himself away from the surgeon until the disease gets very bad indeed. I say, that if a bougie be passed gently and withdrawn as gently, it will produce an increase of the irritation for a time.

The principles upon which bougies should be employed are like those which I mentioned ought to guide us in the treatment of susceptible surfaces. Now I would put as a parallel case the action of a chemical stimulus. What do we do when we have an inflamed eye to treat? First subdue the inflammatory action, and then wash the eye with an eye water. The first application may be horribly painful, but after a time the effect of the stimulus will subside, and the eye feels much more comfortable than before. You desire him to apply the stimulus again and again, until at last he thinks no more of using the wash than if it were common water. Now a bougie is a mechanical stimulus, it gives some pain at the time it is first used, but the urethra is less morbidly sensible afterwards. When you first pass a bougie for a patient, Oh! he will cringe about and complain very much indeed, but after a time he thinks no more of having a bougie passed than of having his hair combed. You are not to repeat the stimulus too much; if you do, you will cause an increased irritation. As much harm as good is often done by bougies being passed too frequently. By introducing bougies too often, you have not only to counteract the irritation which previously existed, but the irritation which you have yourself created. I set down, pass a bougie no more than *once a week*. It produces perfect astonishment in the minds of some persons to find the strictures subside under the use of such apparently trivial measures. If the irritation appears to be increased by the bougie, you must not introduce it again until that irritation shall have subsided, and you must be guided by this entirely. I grant you that the stimulus of the bougie will increase the irritation at first, but if you treat the case in the manner I have pointed out, you will find that it will have a different effect; it is used with the view of diminishing the morbid irritability. They talk of *dilatation*, that bougies are used to cure strictures, by dilatation; as if they could stretch such sensible stuff as the urethra, like as if it were insensible. Now, I say, let a stricture get well without stretching, and that it will do so is proved most pointedly by the enlargement of a stricture round the varnished catheter. This is what they do with strictures in France, they introduce a varnished catheter, and the urethra enlarges around it. I will tell you an instance of this enlargement of the urethra. I saw a man who was making water through six or seven openings in the perinæum. They were fistulæ, and he was very bad indeed. I had brought with me some of these varnished catheters from Paris, and they really were wonderful things, as small as the smallest size bougie, yet perfect tubes. With considerable difficulty I succeeded in getting one of these tubes into the bladder, and the stream of urine that passed through it was as fine as any hair; indeed it could hardly be seen, yet the fluid accumulated in the vessel into which it was conducted. He never made water through it again, for it became plugged up by some means, but he made water by the sides of it, it was left in the urethra for about a week, and one twice its size was introduced. At the end of another week, a catheter twice the size of the last was introduced, and

so we continued to increase them in size until the largest size could be introduced, and the sinus got perfectly well. It is not therefore the stretching of the stricture, but the giving way of the stricture about the tube. I know that here it may be said, that the pressure of the instrument promotes absorption, but I do not believe that it does so. This is the French method of treating stricture, but it is not a permanent cure for stricture.

Irritation of the Urethra.

Dysury, or irritatation of the urethra, depends sometimes on a constitutional malady, and in such cases passing bougies will do very little good; you must remove the constitutional derangement by attending to the state of the digestive organs. Strictures are so very irritable, and grasp the bougies in such a manner, that when you attempt to pass them on, they become reflected and twisted about, and if you persist, you must do a great deal of mischief. In such cases the passing of metalic sounds is better than the bougies. If a sound be introduced gently, it produces all the good of the bougie; the canal enlarges until the urethra becomes of its ordinary calibre. But you will find that it is not an absolute cure for the disease. I have known patients who have gone on from No. 1. to No 20, and yet a little fit of illness has produced a contraction again. However this is what they call the cure by dilatation: I am sure that the cure does not depend upon the frequency of the introduction of the bougies. It was the custom to direct patients to keep in bougies an hour a day, and many old men have I seen who have had a diseased state of the urethra thus kept up for a long time, and which has subsided when this practice was set aside. One of the most striking cases of this kind occurred in an old nobleman, who had very bad strictures, he had bougies introduced too frequently, by which the disease was kept up, and the urethra irritated so much that at last he could only make water in drops. His medical attendants said that he must apply to Sir Everard Home, and get the caustic bougie passed: he did not like the thought of a caustic bougie, and he determined to consult some more eminent surgeon; how he stumbled upon me I do not know. However I discovered that his urethra was in a very irritable state, and told him that I should advise him to bathe the parts frequently with tepid water, and take some mild aperients, to regulate his diet and set his digestive organs to rights; to allow a little time for the irritation of the canal to subside, and that if he would do me the favor of waiting on me again in a week, or if I waited on him, I would then pass a bougie. He did not appear to think much of it, and said, "he thought it was a very nugatory practice." And will your Lordship permit me to say, that you are no judge of it. He was so much better at the end of a week, that I was able to pass a larger bougie than could have been passed for a long time before. I told him to continue the plan I had laid down for him for another week, and that then I would pass a bougie again. Why, said he, my surgeon passed a bougie three times a week, and again observed that it was a "nugatory practice." To which I made the same reply as at first. The state of the canal was so much improved at the end of *three* weeks, that I could pass a larger bougie than had been passed for 50 years. It was not a large bougie certainly, but it was as large as the largest crow quill, and he could empty his bladder with great facility. This is the practice which I should recommend, first soothe and allay the irritability of the urethra, and then adopt the use of bougies upon the principle of treating a morbidly sensible surface, as I have before explained to you, and which I have called the "Surgery of Susceptible Surfaces."

LECTURES
ON THE
PRINCIPLES AND PRACTICE OF PHYSIC,
BY DR. ARMSTRONG.

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LECTURE 18.

In this Lecture I shall speak of the treatment of those affections, the pathology of which I reviewed in my last Lecture.

The Treatment of Acute Muco-Gastritis, or Acute Inflammation of the Mucous Membrane of the Stomach.

When an acute form of inflammation attacks the mucous membrane of the stomach, it generally runs a very rapid course; indeed, if not relieved at the onset, it most frequently terminates fatally. The promptest measures are therefore necessary. There are two means which have great power in arresting this inflammation, namely, *Blood-letting and Opium.*

1. *Blood-letting.*—This must be carried to approaching syncope, else it will do no good whatever, indeed it will do harm. If in an attack of acute inflammation, you bleed so as to weaken the patient, without making a decided impression on the local affection, then I say, that so far from the bleeding being useful, it is prejudicial, inasmuch as it does not remove the inflammation, and exhausts the strength. You must, in this affection, bleed without a reference to any determinate quantity, you must look only to the effect which that bleeding should produce, namely, approaching syncope, and the consequent removal of the inflammation. After the bleeding, the irritation of the stomach being allayed, give a full *opiate*, say from 80 to 100 drops of the tincture of opium to an adult, in a very small quantity of water; or three grains of pure opium in the form of a soft pill. If you have any doubt of the medicine being rejected, if the patient exhibit nausea or retching, then it is better to give the opium in the form of a suppository, or of an injection. It has been said, that three grains of opium introduced into the rectum, have an equal effect to one grain taken into the stomach. But if my observations be correct, the relation is nearly as 1 to 2; two grains introduced into the rectum being equal to one taken into the stomach. If you introduce the opium in an injection, you should use very little water, one or two ounces will be quite enough, for if you inject a large quantity of water, it stimulates the rectum by its distension, and the whole is returned. But the suppository is the better method of the two; and then four or five grains of opium must be used in those urgent cases where copious blood-letting has been premised; but less doses where less blood is drawn. Patients can bear larger doses of opium after blood-letting than they can in health; and you may produce all the good effects of opium where the stomach is irritable, by administering it in the form of an injection or suppository. This plan of treatment, prompt and decisive blood-letting, followed up by a full dose of opium, will very often at once remove an acute inflammation of the mucous surface of the stomach. If any signs of inflammation remain afterwards, particularly under a subacute form, then the application of leeches to the epigastrium will be required. If you reduce an acute inflammation of the mucous membrane of the stomach to a subacute or subdued one, local bleeding will be quite sufficient to remove the latter, provided attention be at the same time paid to the regimen. Twelve leeches at least should be applied to the epigastrium; it is generally better to take them off at the end of a given time, say 20 minutes, or half an hour, than to have the patient worried by allowing them to remain on till they drop off themselves. Sprinkle a little salt water over them, and they will fall off immediately; then put over the orifices a flannel bag stuffed with bran, and wrung out of hot water, and before that becomes cool, remove it, and have another ready, which must be applied in the same manner. This process may be continued for half an hour, and then a little lint may be

pressed on the punctures, and afterwards covered by adhesive plaster spread on linen, in order to stop the further oozing of blood; or a thin warm linseed poultice may be applied, where the weight of the bag is at all oppressive or disagreeable.

If any uneasiness still remain, the leeches ought to be re-applied, and a blister will generally have a good effect, if some chronic uneasiness exist about the pit of the stomach; for blisters, if my observation be correct, have little comparative power over acute or subacute inflammations, but are often beneficial in the more protracted forms. It sometimes happens that a degree of irritability remains even after the inflammation has been completely removed, and then if the stomach be allowed to rest, if it be not crammed with too plentiful a supply of drink, that irritability will entirely abate. Saline effervescing draughts are good in this last mentioned state of the stomach, for they appear to allay the irritability. The food, too, should be of the blandest kind, and given in very small quantities at a time, lest the stomach be offended; the farinaceous articles, such as arrow-root, are the best, or fresh rennit whey will answer extremely well. But I ought to have observed, that in the commencement of the attack, the bowels should be cleared by the administration of an aperient enema.

Treatment of Acute Muco-Enteritis of the Small Intestines.

With respect to the treatment of this affection, it is precisely similar to that which I have laid down for the acute muco-gastritis. Bleed the patient in the first instance to approaching syncope, and, in the next place, as nausea, retching, and vomiting are frequently absent, you may give a full dose of opium by the stomach, having previously evacuated the large bowels by an injection. It is a very important thing to bear in mind, that when you fail in entirely removing the acute inflammation of the mucous membrane of the stomach, or small intestines, that you very often convert it into a subacute form, which frequently, having a sort of determinate duration, goes on for two or three days, or even longer, gradually yielding to a less active treatment than that first adopted. In such cases, the best rule is this, namely, as long as there is any pain on pressure over the epigastrium or abdomen, you must daily re-apply leeches. The subacute inflammation, as indicated by the red-tipt tongue and uneasiness on pressure, may continue for some time, but never consider the patient safe while these two symptoms exist. Nothing appears to me, in reviewing my past practice, more difficult than to ascertain precisely that point where to stop the active treatment, in certain inflammations of mucous surfaces. I am quite sure, that in many of such cases, practitioners sometimes err egregiously, by pushing the active measures too far. If they forget this circumstance, namely, that the subacute inflammation often has a sort of determinate duration, and if they push on the same active treatment, with which they set out, when the inflammation had become subacute, they would destroy the patient. You must adopt an intermediate treatment to remove the subacute inflammation of the mucous membrane of the small intestines.

Sometimes hæmorrhagic reaction, or excitement, follows copious evacuations of blood, attended by that state which, from our ignorance, we call general irritation, some disturbance in the mucous and vascular systems, which, if not properly managed, will not unfrequently prove mortal. Indeed I believe that many patients have died from such a condition, induced by a very copious bleeding, and not rightly controlled afterwards.

Whenever you have reason to believe that inflammation is entirely subdued, and when the pulse continues small, it is best to pause and soothe the patient, to nurse his strength by rest, and not pass at once from one extreme to the other, I mean from the use of evacuants to that of strong stimulants. Life is, in that state, like a fire when it is low; if you throw in a large quantity of fuel suddenly, you will extinguish it, but if you add the fuel gradually, you will support and increase, and kindle it up brightly again. I do not mean to say that there is any real analogy between them, I merely use this language in a

figurative sense, as illustrative of the treatment which should be adopted. You must be very careful, then, what you prescribe when patients have been very much weakened. One great error amongst young practitioners is, that they give too many medicines successively or together. They prescribe something in the morning, and again at noon, and in the evening; they become alarmed, and try to combat every symptom as it arises, and in that manner patients are sometimes worried to death, having no respite from physic or from food either during the day or the night.

There are some *poisons* which produce acute muco-gastritis and acute muco-enteritis, and when we know of no tried and certain antidote, the plan to treat such cases is by common means, the same as you would use in any other excited inflammation. Some persons seem to forget that in the present imperfect state of our knowledge, some poisons must be treated on common principles. I once cured a patient by copious bleeding after *arsenic*, and my friend Mr. POPE, in a case last year, succeeded in the same manner. The person had inflammation of the stomach, he treated that by bleeding and opium, afterwards the head was attacked, and then the bowels by inflammation, each of which he successfully met by prompt and powerful evacuations. Once I saw a young woman who, by mistake, took one ounce of the *impure carbonate of potash*, supposing it to be Epsom salts, and though she had been once bled before I saw her, yet I found that she had all the symptoms, not only of acute muco-gastritis and enteritis, but also of pseudo-gastritis and enteritis. I ordered her to be bled again largely, gave her a full dose of opium, and she got well. Again, I knew a young man who, by way of experiment, took a large dose of *prussic acid*, as many as twenty drops, to try its effects upon himself. He took this in the afternoon, he did all the ordinary business of the shop for nearly an hour, and then suddenly became very sick, and was supplied copiously with tepid water by a friend. I saw him about two hours afterwards, he was then strangely confused in his head, and exhibited symptoms of inflammation of the brain, and lining of the stomach, accompanied by involuntary shudderings and a hot skin. What was to be done? I got him bled copiously at once, and ordered him an opiate after the operation, and he got perfectly well in a short time. Now in this case, if we had waited for some specific remedy, the young man would probably have died. Prussic acid, however, in an over-dose, generally destroys life as rapidly as the poison sold by Shakspeare's starved apothecary.

There are other poisons which endanger or destroy life, by producing inflammation of the mucous membrane of the stomach or intestines. I have seen such inflammation produced by giving large and frequent doses of *colchicum*. A patient had been taking as much as ʒiij of the wine of the colchicum seeds in the day; he had unfortunately continued the medicine after it had produced nausea; and when I saw him for the first time, he presented all the symptoms of acute inflammation in the mucous membrane, both of the stomach and small intestines, in the last stage, and expired within an hour or two from my visit, entirely from an overdose of colchicum. Be sure to observe the rule which I before mentioned, namely, always omit the colchicum entirely, as soon as ever sickness comes on, and then no mischief will ensue. Carefully watch the effects of all active remedies, that they may be timely withdrawn. Never let your own conscience tell you that you have lost a patient from neglect. I have seen other patients in jeopardy from the incautious use of colchicum, but no other case was fatal. This medicine, like some other narcotics, sometimes produces such an extreme shock of the whole system, along with inflammation, that you cannot treat the latter on the ordinary principles of inflammation, in a word, you must sustain the patient by cordials and opiates, while the sunk pulse, the cold skin, and weak respiration remain.

With respect to the different preparations of *antimony*, particularly the tartarized antimony and the antimonial powder, they occasionally produce all the symptoms of muco-gastritis and muco-enteritis, especially when combined with drastic purges, which are still far too fre-

quently prescribed. Here also the general shock produced on the system is sometimes so great that bleeding would be very wrong at first. It is said on the Continent, especially in Italy, that the mucous membrane of the stomach has been found completely blanched in those cases in which large doses of this medicine had been given. But I have seen some cases which have terminated fatally from more minute doses, where the vestiges of inflammation were distinct after death. There is another exception to the use of bloodletting in inflammation of the mucous membrane of the stomach, where it is suddenly produced by the too copious use of *ardent spirits*. The countenance is often, at first, pale, the pulse feeble, and the skin cold, the breathing oppressed, and the pupil contracted. In such cases, the abstraction of the fluid from the stomach should be the first thing, and it may be done very effectually and safely by Jukes' or Read's syringe. Patients frequently die from inflammation suddenly and extensively set in upon the mucous membrane of the stomach. But perhaps the rapidity with which life is destroyed in such cases, may depend on some change which has taken place in the constitution of the blood, and its subsequent effects on the brain and nervous system. It appears from experiment, that ardent spirit is rapidly absorbed from the stomach, as if by a kind of elective attraction.

Treatment of Acute Sero-Enteritis and Peritonitis.

If I were to speak from my own observations on this point, I should say, that the treatment which I have recommended in acute muco-gastritis and enteritis will prove even more successful here. You will recollect that I said, that one decisive bleeding, followed by a full dose of opium, would often remove the acute inflammation from the mucous membranes, or convert it into a subacute form of inflammation. There are, however, some exceptions, cases where repeated general bloodletting is necessary, and this is more especially necessary where the serous membranes are inflamed.

There are, as I before observed, *two stages* in sero-enteritis and peritonitis; the stage of excitement and the stage of collapse. In the stage of excitement you have the hot skin, the quick and resisting pulse, and the local pain; the breathing is quick and anxious, but not feeble. When the stage of collapse comes on, the pulse becomes remarkably feeble, and still more rapid; the respiration hurried, and very weak. The extremities are cold and clammy, and the abdomen blown out, almost like a bladder. Now it is during the first stage that bleeding must be employed, and the effect which that bleeding produces must be alone regarded. It is in vain, indeed absurd and dangerous, to lay down any precise rule as to the quantity of blood that should be drawn in such cases as these; we have not yet arrived at that arithmetical accuracy which can enable us to say that in any given cases, ten, twenty, or thirty ounces of blood shall produce such or such an effect. There is no proposition more frequently rung in my ears than this, that a certain quantity of blood must be drawn; but I say that this is an arrant sophism, a mere routine fallacy, with which practitioners may not only hurt their reputation, but fatally affect others. I have repeatedly had occasion to caution you against indiscriminate copious bloodletting in inflammatory affections; but in cases like the present, half measures only hurry the patient the more rapidly to the grave. I protest most strongly against half measures where a few hours must determine the life or death of a fellow-creature. You must here, in bleeding, not be swayed by doubt or indecision, you must not have your judgment subdued, or your hand unnerved by the restrictions of schools or colleges; you must not, I say, look to the quantity, merely, of blood drawn, but regard, I repeat, the effect, and the effect only. If you find that after you have drawn ten, fifteen, twenty, or even thirty ounces of blood, the patient is not brought to approaching syncope, go on steadily till you produce that effect; do not, I beseech you, stop short in the middle; proceed boldly in the good work, and the general result will raise your character in the public estimation, for, aided by subsequent means, then

your practice will be highly successful. In such examples, I can most truly assert that I hardly ever lose a patient when seen at the commencement.

After you have bled the patient to approaching syncope, give him a full dose of opium, at least three grains, in a soft pill, as soon as ever he recovers from the faintness; and I do say, on my honour, that I have never seen a practice which, in its success, was at all comparable to this, repeated if necessary. You should have the opium ready, in the form of a very small draught, containing 100 or 120 drops of the tincture, or a soft pill, containing three, four, or five grains, so that as soon as the patient rallies from the shock, you may give it to him, and then leave him perfectly quiet, for two or three hours. Let him be quiet and he mostly falls into a sound sleep. On visiting him, you will often be delighted to find his skin warm and moist, his tongue moist, his pulse soft, no pain, in fact all the signs of convalescence. But it sometimes happens that you have occasion to bleed again; but do not, on any account, wait longer than three or four hours; that, indeed, ought to be the longest period which you ought to wait, and if you then find that the skin is hot, the pulse quick, the breathing hurried, and that there is pain on pressure, you must bleed in the same decisive way as before, and give, after the second bleeding, about two grains of opium with two grains of calomel. Visit the patient again at the end of two or three hours, and bleed again if any signs of inflammation exist, and afterwards give one grain of opium and two grains of calomel. It is scarcely ever necessary to bleed after the third time, in the most severe forms of serous abdominal inflammation, if you follow the plan I have directed, and far more frequently, only a first or a second operation will be requisite, so that the quantity of blood drawn under this plan is ultimately less than that taken ineffectually in small repeated bloodlettings. Do all that is to be done in eight or twelve hours, and not occupy forty-eight hours, or more, for the purpose, as is too frequently the case. Treat the patient as mildly as possible afterwards.

There is one circumstance which I wish you to attend to particularly, respecting the use of opium, namely, the state of the tongue. I have never found opium beneficial except where the tongue was moist; if you put your finger on the tongue and find that there is a degree of moisture left upon it, then you may expect the opium to be useful after venesection; but if you find the tongue dry and glazed, as it is in some specific fever, opium will never be of much benefit, especially if the brain be consentaneously affected. Where large quantities of blood have been lost, hæmorrhagic reaction, or excitement, often follows, and opium is extremely useful in preventing the occurrence of that condition which is so apt to renew the inflammation. In hæmorrhage, after delivery, and in all cases where patients have bled much from accidents or operations, opium is of the greatest use in this way. The effects of opium, in the affection which I am at present considering, appear to be those of preventing the return of the hæmorrhagic reaction, and the return of the pain; besides, it acts on the skin by inducing diaphoresis, while it causes the patient to fall into a quiet and refreshing sleep. If you do not give opium after bleeding the patient to approaching syncope, when you return to him in the course of an hour or two, you find that he is worse than before, the hæmorrhagic reaction having been established and the inflammation aggravated thereby. Before I knew the good effects of large doses of opium I lost many patients in acute inflammations, although I bled them copiously enough.

But what are we to do with *purgative medicines?* I say, generally, do not give them at all until the inflammation be removed. In the early part of my practice I gave them, because I was directed to do so, but since I have trusted entirely to my own observation, I have given up the use of purgatives altogether, until the inflammation is removed, except in those cases where the colon is surcharged with fæces at the time of the attack, and that I remove, in the first instance, by the administration of a large aperient enema, which is then, certainly, often an excellent auxiliary. Constipation is the effect and not the cause

of inflammation of the serous membranes of the bowels; and, except in the case just mentioned, it is of no importance if the bowels be not opened for the first day.

The late Dr. SAUNDERS used to say, emphatically, "open the bowels by the lancet;" and certainly, in these cases, there is no purgative so good as the lancet. If they do not act under the influence of bleeding carried to relaxation, they will frequently do so with a little cold drawn castor oil, after full doses of the opium; the last of which frequently acts as an aperient in examples of this nature, its operation being thus modified by the condition of the body at the time of its administration. But also recollect what I have already said in regard to injections. Sometimes I have been called to females labouring under this affection who have experienced a great desire to go to stool, but who could void nothing; I have examined the rectum, and generally found it loaded with hard dry fæces; these have been removed first by the handle of a spoon, and next injections have been used till the colon completely delivered its contents. I have known small repeated doses of calomel relieve the bowels in such cases when common means failed. I mentioned before, that in all cases of seeming enteritis, you should make a point of ascertaining whether or not patients have hernia; for in the course of my experience, I have known more than a dozen cases in which the hernia had been overlooked, and the patient unfortunately treated simply for enteritis. A man was sent into the hospital, supposed to be labouring under low fever; he had vomited daily for nearly a week, and during that time he had not had an alvine evacuation. On examination, I detected a hernia. Mr. WALNE performed the operation very skilfully, and though the intestine, which had been strangulated, was, when exposed, almost as black as a hat, yet the patient recovered very well.

Peritonitis requires the same treatment as sero-enteritis. The great difference between the peritonitis of child-bed and common peritonitis appears referrible to the irritable state of the nervous system, and to that more tender condition of the abdomen, so that the inflammation generally runs a more rapid course, and requires correspondent firmness and activity of treatment. If the first six or eight hours be lost in such instances, life is most frequently lost; but if that time be properly employed, nineteen cases out of twenty will recover, if the subjects be organically sound before the attack, and if the treatment be prompt in the first instance and proper in all respects throughout.

There are some cases of peritonitis perhaps necessarily fatal, such as arise from *ulceration* of the mucous membrane of the intestines, which, as soon as it touches the peritoneal coat, brings on an acute form of inflammation, which commonly runs a fatal course in about 48 hours; nothing stops its progress, as far as I have seen. There is another cause of peritonitis which is generally fatal, but not necessarily so, it is a *tubercular state of the peritoneum*. I may relate a case to you of this kind:—A child became very unwell, his face pallid, he wasted very much in his extremities, and his belly grew large; he had occasionally uneasiness when he passed his motions. By advice the father sent him to the sea-side, and there he seemed to improve a little, but he returned in a steam boat, the motion of which alarmed and sickened him; he, however, reached home, which was a little distance from town. His parents, being alarmed, sent for me, but he died before I reached the house. Suspecting tubercular disease from the past history, and wishing to ascertain the cause of death, that if possible I might instruct myself in order to be more useful to the living, I requested an examination, which was granted, as the intelligent parents had other children whose lives they of course valued most dearly, and who might, by possibility, be liable to similar affections, which once really known might be prevented from attacking the survivors. When the finger was passed lightly over the surface of the abdomen, little knots could be felt, which I suspected were tubercles on the peritoneum. On further examination,

I found hundreds of these tubercles on the peritoneal surface. They do not, in the first instance, generally appear to be dependent on inflammation, but they often excite inflammation ultimately. But though many of these cases are fatal, yet I have known some recover, and recently I saw a lady who had, apparently, these tubercles connected with inflammation, but who recovered from the latter, and now remains tolerably well. You must, in these cases, give only the mildest aperients.

Treatment of Subacute Muco-Gastritis.

This is a form of inflammation which is very common. I once attended lectures at a college where the professor said, that this was a very rare disorder; but I have found it not uncommon, and facts are more powerful than the assertions of professors. You must leech the epigastrium; put on twelve leeches, or more leeches on the first day, and open by very mild aperients, as gr. iss. of calomel with gr. iv. of rhubarb in the day, followed by a small dose of cold drawn castor oil, or infusion of senna with manna; at the same time allow only the blandest diet. You must repeat the leeches daily till the pain be entirely removed; that is the best general rule. I would say, the same treatment should be adopted in the subacute muco-enteritis. I do not recollect that I have lost a case of subacute muco-enteritis when I have seen the patient early; yet I have seen several cases terminate fatally where they had been neglected in the outset or progress, and on examining the bodies after death, have most frequently found ulceration in some portion of the ileum. The French physicians now mostly condemn the use of aperients in inflammation of the mucous membrane of the intestines; but I have found the mild ones already mentioned productive of much good.—When, however, any irritation is excited by them, they should be omitted, and especially if hæmorrhage take place from the mucous surface of the intestines, for then nothing is more injurious than the employment of purgative medicines, while that of opium is almost a sovereign remedy in conjunction with absolute rest and a light diet.

Treatment of Subacute Sero-Enteritis.

Early general bloodletting, succeeded by local bloodletting, opium, enemata, and gentle laxatives, with a strictly antiphlogistic regimen, are the best measures. In all abdominal inflammations, it is better to avoid the early application of blisters, for then they are not serviceable, and by their operation on the surface you lose the test of the presence or absence of the inflammation, namely, pain, or the contrary, on pressure; but blisters are useful if the inflammation put on a more chronic appearance.

Treatment of Inflammation of the mucous membrane of the large Intestines.

The disorder called *dysentery*, is an inflammation especially seated on the mucous membrane of the sigmoid flexure of the colon and upper part of the rectum; sometimes, however, it extends into the mucous membrane of the small intestines. Occasionally there is a high degree of fever, and great local disturbance; yet the patient is often chilly in the onset, and if you see him then, put him into a hot bath, and give him a full opiate afterwards; the attack often terminates by a copious perspiration. But if you are called after the stage of excitement has taken place, the pulse hard and quick, and the skin hot, you must bleed the patient at once to approaching syncope, and afterwards use local bloodletting by leeches, which generally has a most excellent effect. There are some cases of an intermediate form, in which the pulse is not so quick or the skin so hot, nor the local disturbance so distressing, as in the extreme form. I had, in October last, an attack of this form of dysentery, my pulse ranging from 80 to 100, but the tormina and tenesmus were severe and frequent; I was advised to try calomel and opium till the mouth became affected; it happens that calomel sometimes produces the most profound relaxation, which is worse to bear than pain; it did so in my case, and I requested that it might be given up on that account. I was

bled repeatedly by leeches, which were applied as many as five times over the abdomen, and at each time with the greatest relief. At the instance of my experienced friend, Dr. AYRE, I used an opium suppository; before I did so I was passing mucous stools about every half hour, and afterwards I only passed one in eight hours. It was curious, in my own case, to observe how the balance of the secretions was lost and restored; at first, and for some days after the attack, the secretions of the liver seemed suspended, and those of the skin and kidneys much diminished, while the work of the mucous membrane of the large intestines was greatly increased, as the copious laxation of mucus mixed with blood showed; but as the affection of the mucous membrane of the large intestines abated, the secretion of bile became copious, the skin became moist, and there was a plentiful flow of urine.

It has occurred to me to see many cases of dysentery, before and since last October, in London, and I have found that repeated leeching, with full doses of opium introduced into the rectum under the form of an enema or suppository, is the most successful plan of treatment, aided by a little cold drawn castor oil occasionally. Leeches seem to have a decided influence on this inflammation when used early, and repeated as long as the pain remains. The common method of treating dysentery is by exciting ptyalism; the cases which occurred in the Penitentiary were treated in this way, and generally speaking, it answers remarkably well. But I should now be disposed to adopt, preferably, the mode which I have just mentioned; but if I found the disorder did not yield to it, I would then affect the system rapidly by calomel, or some other mercurial.

There is one form of dysentery in which the fever is very often masked, the patient is greatly oppressed, the pulse being small and the breathing rather weak; but if you put your hand over the belly, you find that there is a concentration of heat there, though the extremities be cool or even cold. In this form of the disorder, there is generally an hepatic and a bronchial affection combined. If you bleed in such cases largely, they will terminate fatally, and you must, therefore, treat them by leeches, by opium, and calomel, aided by a gentle laxative now and then. I was informed, by an intelligent pupil of mine, that an epidemic dysentery prevailed about Nottingham, and that those cases in which large bleeding was used mostly proved fatal, while those treated by leeches, calomel and opium, recovered; such a form of dysentery is denoted by a concentration of heat over the lower part of the abdomen, by a soft compressible pulse, by the great prostration of strength, and by the heat on the surface of the other parts of the body being rather low.

In all cases of dysentery, the strictest attention should be paid to the diet, for if you were to allow a patient to cram himself with every thing he desires, he would be almost sure to die. Be very careful to avoid the skin, husks, and seeds of fruit. When the stools are becoming fæculent, you may hail it as the first indication of recovery; these appear, at first, like bird-lime, or meconium, and all you have to do is to give a little cold-drawn castor oil, still regulating the diet attentively. If you fail to arrest the progress of the dysentery by local bleeding, repeated as long as the pain continues, and by opium, if you then affect the mouth by calomel and also fail, the affection generally passes on to ulceration, which is found in many places after death, with considerable thickening of the whole of the affected intestines. But be prompt and judicious in the beginning, and your success will be great in this disorder, as it occurs in this country.

[ERRATA in Dr. A.'s last Lecture.—Page 263, first column, for "tint on the surface," read "*heat* on the surface;" p. 267, second col., for "great irritation," read "great *emaciation*;" and for "what gives us," read "what *he gives us*;" p. 268, first col., for "I have known them descend," read "I have known them *diseased*."]

REVIEWS FROM THE WEST.

Westminster Review.

Art. "Contagion and Sanatary Laws."

STATIONERY to no end has been expended upon this intricate subject. Oceans of ink have been shed in fixing the colour of contagion,—the cameleon of the medical world. Yet are its hues as far from being determined as ever, borrowing, seemingly, a novel tint from every medium through which it may be reflected. On land and on sea; in the camp, the fortress, and the lazaretto, has it been followed by the same unsubdued spirit of philanthropy. Well may it be said of this scourge of the human race:

"For this hath Science search'd on weary wing,
By shore and sea, each mute and living thing;
Launch'd with Iberia's pilot from the steep,
To worlds unknown, and isles beyond the deep!"

Happy should we feel if the excursions of philanthropists were always crowned with success, but now, unfortunately, they have become proverbial for failure. We can well appreciate the humanity of their motives, though we cannot be blind to the generous errors of enthusiasm. At home and abroad, the subject of contagion has been unceasingly discussed, and still are we sceptical! Has the experimental death of young White had no effect on the minds of those who hold the plague to be a non-contagious disease, or the more fortunate escape of Maclean not disturbed the faith of those who hold an opposite opinion? Inconsistent human nature! while we are in the act of censuring the conduct of others, we too, have mended a pen, and drawn forth a sheet from our *escrutoire*, to add our mite in setting at rest this eternal theme of contention. Hopeless attempt! to make that intelligible which we cannot comprehend. Without, however, affecting to be wiser than the rest of mankind, we may take a seat and gossip awhile amongst the crowd. Where so many disagree, singularity should not be dreaded. We sit down to our task with a certain conviction of being laughed at by one half of the medical world, and of being applauded by the other, perhaps not from motives the purest in either. Every man has a right to express his sentiments. No man, though he should possess the power, has a right to enslave the minds of others by ingenuity. For one man to construct a pit that his neighbour may fall into it, may be very amusing to a looker-on, but not quite so agreeable to him who risks his life in the experiment. We are not partial to practical jokes, whether played off on our body or our understanding. These reflections have been excited by a haberdasher in points and particles, who attempts a trick of this kind in the last number of the "Westminster Review." He rounds his definitions with the accuracy of a compass, and thinks he compresses the whole subtle essence of contagion within the narrow limits of the magic circle. From these self-created postulates, as from an armoury, he draws forth such conclusions as best suit his purpose. He selects, from the contents of the book of nature, such facts only as incline to his own side of the question—he sees nothing there to militate against his arguments. Nature would seem to labour only to furnish him with weapons to wield at will against the contagionists. He sits down, shuffles the cards, chooses his own hand, and wins the game, of course, without the honours of a victory. The automaton whist-player is a fair illustration of this species of argumentation, in more points than one, for the sage Reviewer would impose silence on all the world, while he reserves an exclusive right of talking to himself. Now we hope to be excused if we protest against an important subject being disposed of by a dextrous combination of *moves* on the chess-board of sophistry, or having it gambled away, like an estate at a club-room, by the agency of palming and false dice. We can very well bear to be told that two and two are four, but when this mathematical certainty is extended to matters that do not readily admit of absolute demonstration, the mind instinctively spurns the trammels of logic, however artfully woven or fashioned to sit easy on the captive. Such is the manner in which this writer treats contagion. He defines, divides, deduces, and dogmatizes; and while he denies medical men the right, or rather the capability of judging in their own concerns, he takes from amongst this incompetent body a Doctor Maclean, and, with this same Doctor, knocks down the whole fraternity. This is but a poor specimen of his consistency, for he roundly asserts that contagion is not a question exclusively medical, but one of science, to be decided by facts. We would be glad to know what question, among medical men, is not decided by facts? and we would be also happy to know who are so capable of judging of facts as those

who are daily conversant with them? but the best of this joke is, that ninety-nine out of a hundred parts of his evidence are derived from the tainted source of medicine. Having thus glanced at the manner of this writer, we shall state his objects, and the means which he has adopted for their accomplishment.

By some chance or other, mankind have been in the habit of looking on typhus, the yellow fever, and the plague, as contagious diseases. The "Westminster Reviewer" declares, without reservation, that they are not. To make good this unqualified assertion, he commences by defining the terms contagious, epidemic, and sporadic; for under these three epithets he classes the whole family of human maladies. A contagious disease, he says, is a disease communicable from one person to another, and depends on the operation of a specific animal poison. An epidemic disease is caused, or is supposed to be so, by a certain constitution of the atmosphere; and a sporadic disease is one which may happen when or where it may. We regret not being able to lay before our readers the entire of these learned definitions, in the writer's own words; but our shallow bark would not bear to be burthened by the heavy ballast of the "seventy-four" that has lately hoisted the "broom" at its mast-head, and threatens to reign supreme over the realms of literature. Narrowing the sphere of discussion, but still fondly clinging to the principle of division and definition, he discovers that all diseases are separable by two adjectives—acute and chronic: and, of course, contagious diseases must observe the same law. Accordingly, he takes the hint, promptly distinguishes contagion into acute and chronic, points out variola and rubeola as standards of the former—syphilis and psora, as illustrations of the latter. The acute contagions are propagable at a certain distance: direct contact is necessary to the production of the chronic. Both these forms of contagion, though differing each from the other in many respects, agree in one point—the observation of certain laws. These laws he finds to be determinate—they never vary. The accession of the symptoms is regular—the symptoms themselves, in any given number of cases, never differ except *in degree*. The *virus* of variola produces its appropriate symptoms, so does that of rubeola; the one never produces the other. When once the specific matter of these diseases is secreted, it can be propagated to any number of individals, and at any time. Every thing, in short, about contagion is certain, except its origin, which, being an ultimate act, is not to be scrutinized. Having thus dressed up contagion in an armour best suited to fight his battle, he confronts it with an epidemic. Here we find every thing as uncertain as they were certain above; for, an epidemic being the offspring of the atmosphere; it is only natural to suppose that it should, in some measure, partake of the versatile movements of its elemental parent. And so it does; for he represents it as a downright weather-cock, veering at one and the same time to all the points of the nosological compass. It is here to-day and away to-morrow, a sort of will-o' th' wisp, or *ignis fatuus*, without a local habitation wherein to rest. The symptoms of this creature of chance, as might be expected, he describes to be as unstable as its origin and progress. Being thus independent of time, place, and circumstance, this amorphous creation might be best defined, like Milton's demon, by a negative of qualities, "if shape it might be called that shape had none:" so, in like manner, having no form, the absence of fixed laws must be considered the rule by which this antinomian monster is regulated. Such is the relation which an epidemic bears to contagion, as stated by this writer; in the one all is as regular as the course of the planets, in the other, irregularity is the predominant feature. Matters being thus judiciously arranged, it was time that the author should conclude the process which he had so artfully conducted, and the conclusion which he draws from the whole of these premises is, that things so totally dissimilar in every respect, were incompatible with each other. It now comes to our turn to speak: and, we trust, our readers will at least give us the merit of honest intentions, when we assure them we have done all in our power to put them in possession of the substance, the drift, or the what-he-would-be-at, of this writer's discourse. For after all this defining, deducing, and dividing, the question resolves itself simply into this—Are there facts to prove the contagious nature of typhus, yellow fever, and the plague? This is really the question to be decided, unincumbered by a parade of learning, and un-bewitched by the legerdemain of logic. We will not content ourselves with showing the possibility of these diseases being contagious, but that they are undoubtedly so; and as he has been at considerable pains to establish the truth of his opinions, by a preparatory process of reasoning, we shall run over the whole series of his labours, *seriatim*.

In the first place, our readers will perceive that the trinity of epithets, contagious, epidemic, and sporadic, being all applicable to the same disease, cannot serve as the basis of nosological distinction. What is predicable of the first term, is also predicable of the other two; thus we might with perfect propriety say, a contagious epidemic; an epidemic contagion; a contagious sporadic; or a sporadic contagion, and so on. And why not? The disease that is contagious to-day, in a short time becomes epidemic; and what is an epidemic now, at no distant period must become a sporadic; nay, in different places, at the same hour, a contagion will be both epidemic and sporadic. In this sense, I believe all the world has understood these terms, merely as qualifying something, but without themselves constituting any qualities. And was the world in error in so doing, under the circumstances we have just stated? Certainly not. If the nomenclature of nosology be defective, that can be no reason why its imperfections should be converted into data for the construction of erroneous conclusions. The absurdity of arbitrary definitions could not be more clearly exemplified than in the instance just before us. The division of diseases into acute and chronic, is but a less felicitous specimen of the art of saying nothing while the writer's pen is going at the rate of twenty knots an hour. True, these terms are common in the medical conversation, but we believe it is the first time that they have been employed to bisect the circle of human maladies. We say so because these terms are extremely vague, admitting of various interpretations; and, even in their ordinary acceptation, totally inapplicable to many diseases with which we are acquainted. Generally speaking, the first of these terms is understood to relate to a certain morbid condition of parts; the second, to a continued and lesser degree of that morbid condition. In fact, these terms have been universally employed to designate certain stages of inflammation—in the mind of every man they seem to be connected with some certain state of a part or parts of the human body where inflammation is supposed to be present. How then are we to dispose of a numerous list of diseases which do not depend upon inflammation for their existence? We need scarcely call upon mankind to attest in how many ways they may be tormented without the agency of inflammation at all. Perhaps the Reviewer would deign to take this small fry of diseases under his gracious patronage, in his next classification. If then a division of diseases into acute and chronic is untenable, what shall we say of a division of contagious diseases into chronic and acute? We did, indeed, think that this bold attempt was reserved for some man of forlorn hopes, who having failed of attracting attention in the usual manner, was willing to give up all pretensions to consistency and common sense, for the attainment of notoriety. But let us just see how this tale hangs together. Variola and rubeola, we are told, are acute, syphilis and psora chronic contagions. And for what reason? Because, says the sage Reviewer, the former affect the same person only once, the latter oftener. Nothing can better show the indefinite sense in which these terms are used; for here we are informed that a disease is acute not because it is attended by any inflammatory state, but because it affects the same individual but once in his life. Nosologists have, 'tis true, applied the term *acute* to certain "exanthems," not on account of affecting the same person only once, but because these diseases were accompanied by a train of symptoms which they fancied to be, or confounded with inflammation and fever. We have, on a former occasion, in this Journal, shown that fever and inflammation are not synonymous terms. If this be true, and no man can doubt it, why should the term acute be applied to a state of the system of which we are ignorant. No person, we believe, ever applied the term acute to fever; how, for instance, would it sound in the Reviewer's ear, to hear a man say, "there, Sir, is an acute typhus fever, or an acute hectic fever, or a chronic intermittent." Any man, indeed that would call a tic douloureux or an amaurosis, of nine or ten years' standing, by the name of acute or chronic, might, we think, be laughed at without committing a breach of charity or of good manners. Now, if we allow these diseases to be called acute, (fever, for instance,) we must allow them to be also called, after a certain duration, chronic. There can be no greater abuse of language than this practice would lead to. We should be glad to know the precise hour at which it would be lawful to call an intermittent a chronic disease; for we have seen cases of this malady at the end of three months, precisely the same, to all appearance, as the first week they attacked the patient. We must entreat the reader's indulgence for detaining him so long on a matter of so little interest, and the absurdity of which he must have perceived without our assistance. We shall therefore say no more upon that part of

the subject, but proceed to examine further the propriety of a division of contagious diseases into chronic and acute. It is obvious, if diseases at all admit of being divided into two great classes, such division must depend upon some strongly marked symptoms peculiar to each class. We presume it must have been upon this principle that the Reviewer proceeded, when he marched all our ailments to the right and left, under the respective colours of chronic and acute. But how soon does our pathological drill-serjeant loose sight of consistency, for now that it suits his purpose, he marshals the contagions into two battalions, without any respect either of colour or complexion. If any disease can be justly termed acute, that disease is scarlatina; yet it affects the same person more than once in his life. In what class, therefore, shall we place it, to the left or to the right? Since it will not harmonize with either of the Reviewer's classes, we would beg leave to suggest to him the propriety of placing it amongst the *hic, hæc, et hoc* genus of diseases, or it may be put down, like some syllables in prosody, as doubtful. "Quo neque, longa, brevis, natura dicitur anceps." But to lay aside these puerilities, why should variola be considered an acute disease more than syphilis? Has not syphilis its regular period of accession, its pathognomonic symptoms, constitutional fever, eruptions, &c. all as regular as variola? Shall we call pertussis the "bex convulsiva" of Good, acute or chronic? It affects the same person but once, its contagion is propagable at a distance: but without either vesicles or pustules, or any specific animal poison cognizable to our senses, we cannot therefore propagate it at any time, or among any number of individuals we choose; a character, as we are told, of genuine contagion. We have no objection that the term acute should be applied to contagious diseases when symptoms would warrant its use, but to call that disease acute, the subject of which we see running about the streets with his playmates, is, we think at least, very ridiculous. Were we to run over the history of the various contagions—show in how many points they differ and agree, how variable, nay contradictory, they would be found in many respects; we might more fully justify our protest against such a division of contagious diseases, but we should, at the same time, commit an outrage on the reader's patience, and a wanton waste of our own time.

We shall now proceed to examine whether that great difference exists between epidemic diseases and contagions to justify the conclusion of their incompatibility. The Reviewer lays much stress on the diversity of the symptoms of epidemics, the doubtful period intervening between the exposure to their influence and the appearance of their effects, their rapid progress and decline, their dependence on the state of the atmosphere, their modification by climate and the constitution of the patient, their want of pathognomonic symptoms—in short, their total dissimilarity from contagion, and a hundred other such silly expressions, to which we may reply by asking him: Are contagious diseases really independent of atmospheric influence, unmodified by place, climate, and constitution? Yes, says the Reviewer, "a little *in degree*." Oh! but for that *degree* all would be well. Is not variola known to prevail more at some seasons of the year than at others? Is not the period of its epidemic existence as uncertain as any other uncertainty with which we are acquainted? Is not its cessation a matter just as much beyond our comprehension? Like typhus fever, variola is constantly amongst us, but, like typhus, its awful visitations are irregularly periodic. If variola has its pustules, has not typhus its petechiæ, has not the plague, in like manner, its buboes, carbuncles, and vibices? If the matter of the variolous pustule communicates that disease, has not the matter of the pest buboic done the same, not only to men but also to dogs? Was it candid, we ask, in a writer discussing a subject in which the lives of thousands were concerned, to leave all this evidence out of sight? Nay, he has not only suppressed the truth, but his blind impulse to carry all before him, *per fas et nefas*, has induced him to state what is not the fact, that you can propagate rubeola from its vesicles. This, as every person knows, was attempted by Home, of Edinburgh, and we are told, that half those whom he inoculated did not become affected by rubeola at all. Now, it was not from the vesicles of that disease he sought to propagate it, but from the blood and ocular secretion of the patients. In fact, we believe it to be impossible that the disease can be propagated from the vesicle, for, if it could, Home and others who failed altogether in their experiments, would not have had recourse to the patient's blood, or the secretions of the eye. Contagions are not quite so regular as one would be led to expect from the statement of the Reviewer. Of variola, medical writers de-

scribe three forms—when produced by inoculation the eruption appears about the seventh day; when the disease is naturally contracted, the term varies to fourteen days and upwards; whence this difference? Rubeola is still more uncertain in its symptoms, varying from an ephemeral pyrexy to the worst possible shade of typhoid pneumonia. Why should these diseases prevail more at one time than another, unless, like the barometer, they are under the influence of the atmosphere? On other occasions, we find contagions making their way against all opposition from the elements. The spasmodic cholera of India leaves the steady force of the monsoon, and moves with the rapidity of lightning, unobstructed by salubrity or temperature of climate. There are countries, where, we are told, that syphilis is quite friendly to the natives, not requiring the use of mercury for its cure, while to strangers it is absolutely destructive. How many kinds of syphilis have we not? Mr. Carmichael can answer this question. The Hunterian chancre is giving way fast to a whole tribe of syphilitic sores; the *pseudos* we think, will give the classifiers something to do very shortly. We need not follow this line of argument longer; satisfied with exciting in the reader's mind that train of thought necessary to pursue the speculation at pleasure.

Let it be admitted, for argument's sake, that contagious, epidemic, and sporadic diseases are just what the Reviewer described them to be; that diseases are separable into two classes, chronic and acute; that contagions observe the same law; that epidemic and contagious diseases are as different in appearance from each other as limestone is from a diamond; how far does all this go to prove that the plague, typhus, and yellow fever are not contagious? As well might he say, that syphilis was not contagious, because its symptoms did not resemble those of variola; one conclusion would be as near the truth as the other, for both being founded on an assumption, prove nothing.

We now come to the consideration of the origin of contagion, and shall quote two sentences which we believe contain the whole essence of the Reviewer's logic upon this head:—"The spontaneous generation of a contagious disease is as great an absurdity as the spontaneous generation of an animal. To suppose, indeed, that a disease, non-contagious in its commencement, may become contagious in its progress, is to imagine the same thing in the animal economy, precisely the same absurdity as it would be in the vegetable, were an acorn, by change of climate or of soil, to cease to produce an oak and generate a bramble." We suspect the Reviewer, when he writ this, must have been after taking Sir Morgan O'Dogherty's *quantum sufficit* of "toddy." Had he been in the "blue parlour," along with Tickler, that critical cannibal might have asked his Reviewership—"You cut-throat Whig, if contagions are not spontaneously generated, did they produce themselves?" The Westminster Doctor might refer him to Pandora's box for a solution of this interrogatory, for on no other principle, according to him, can the existence of contagion be accounted for. To say that contagion is co-existent with men and animals, a conclusion deducible from the Reviewer's logic, would be only to say what a man might be justly sent to the madhouse for. One thing, at all events, is certain, that such things now exist, and it is almost as certain that they did not produce themselves. How, then, were they produced? They were not self-created, for that would be giving to them a power which we reserve for Heaven alone. Let us look to medical history, and invoke its aid upon this obscure subject. The earliest written accounts which we possess of variola are those given by Rhazes, and do not extend farther back than the seventh century. All agree that syphilis is comparatively of modern origin. Even Sir Walter is silent on the origin of Scotia's national disease, though it is probable some of his heroes and heroines played, at a very early date, on the "Scotch violin." But let us suppose them to have existed as long before as they are known to us; let us even transcend that "watermark" on the abyss of time, and boldly enter Paradise; shall we find there the germs of all contagion implanted in the first of men by the hand of his Creator. It is folly, nay, it is wicked, to suppose that such can be the origin of contagion. There is no other means left us of resolving the "Gordian knot" of this difficulty, except *spontaneous generation* in the human body. That those contagions now amongst us were spontaneously generated in the bodies of persons, is as certain as that those diseases exist; of the time, place, and circumstances we are ignorant; what happens once, may it not happen again? But why resort to any process of reasoning, or even consult the page of history, when daily experience must convince every man of the spontaneous generation of specific animal poisons. What medical man, in any practice, has not

seen cases of spontaneous tinea capitis, where no suspicion could exist of the disease being communicated by contact? Who has not seen isolated cases of variola where, within miles round, there was not another case to have disseminated the contagion? Who, in like manner, has not seen cases of rubeola and scarlatina occurring idiopathically? Nay, if we can believe the solemn asseverations of men reputed honest, we must believe in the occasional production of syphilis without sexual connexion. From men let us pass to brutes. We can hardly think that the Reviewer could have lived to this time of day without hearing of such a thing as a mad dog. We tremble for the consequences of putting the question of spontaneous generation to him here, for if he possess the common feelings of a man, he must perform a quarantine of double the usual period, to blot out the "plague spot" of ignorance or dishonesty from his character. He, indeed, talk of contagion and specific animal poison, that could not discover—that had not eyes to see the *spontaneous generation* of both take place in the mouth of a dog! Let him only adopt the following suggestions, and he cannot fail of being convinced of the *spontaneous generation* of various specific animal poisons: seclude a number of dogs, keep them together in filth and a state of starvation for six months, and he may rest assured that some of them will generate the *mange*: observe the same rules with any number of lambs, and before they are sheep he may be certain that some of them will generate the *scab*, and we need not add that one scabby sheep infects a flock; to produce the *glanders* in horses, he has only to maltreat and keep them in bad air; and if the Westminster Reviewers wish for a personal proof of the spontaneous generation of a contagious disease, they have only to huddle together in filth and poverty for some time, like so many Highlanders, and we will ensure them, that one or other of the party will generate as firm a specimen of the *itch* as ever the "Land o'Cakes" yet produced. The truth of the matter seems to be, that it is in the nature of animals to generate contagious diseases, just as it is in the nature of the Reviewer to be inflicting his paradoxes on the world. We are told by him, "that a disease, non-contagious in the commencement, cannot become contagious in its progress." Now it so happens, the first writers who treat of variola, Rhazes, and Aoran, represent this disease not contagious at all, while it affected the same persons twice and oftener. Has it then become contagious in its progress, and totally altered in its nature? If it had been a contagious disease, it could not surely have escaped the observation of these writers. From their opinions on these points, it is not too much to infer, that since that time it has undergone two great changes—changes which prove (if other proofs were wanted) the spontaneous generation of contagion capable of affecting the same person but once in his life. To suppose that a human body, to us apparently in perfect health, can produce any disease at all, and not produce a contagious one, is to swallow a windmill and be choked by a fly.

From what has been advanced here, it will not require much faith in our readers to believe in the spontaneous generation of contagion, though they may not believe that animals were produced in this way. To incumber our pages with facts in testimony of the contagious nature of plague, &c. &c. would be only to do that which has been performed by much more competent hands; we will therefore remain satisfied with overturning the doctrine of this writer upon his own principles. By the substitution of exceptions gleaned from every source, he has sought to overturn the well-known laws of contagion. By following his example, it would be no difficult task to prove those diseases, which he acknowledges to be contagious, not to possess any such quality. We shall adduce but a few examples, which we are confident this writer, upon his own principles, must take as a refutation of the contagious nature of variola itself; we allude to the experiment performed by WALSHE. This gentleman procured a table of certain dimensions; had cotton, charged with fresh variolous matter, placed in the centre; children, in perfect health, were next seated round the table for, we think, an hour; they were then removed, but at the regular time of the accession of variola the disease did not appear. Various distances were then tried, but without success: a child, covered over with the eruption of variola, was substituted for the cotton, as the focus of contagion, but, strange to tell, (if we recollect rightly,) at the end of nine months, that one of those children spontaneously took the disease! Now we ask the Reviewer, candidly, Would not this experiment—must it not, on his own principles, have convinced him that variola was not contagious? Had a plague patient been on the table instead of the little urchin, what a triumphant

refutation it would afford him of the non-contagious nature of the plague!

We have exhausted our space, but not the subject of "Sanatary Laws." As soon as this writer shall speak, we promise to follow him, as we have done at present. We cannot, however, but remind him, that however interested he may feel in the welfare of commerce, the public ought never to sanction the abolition of preventive laws as long as a shadow of doubt exists on the conveyance of contagion in the hold of a ship; and justly, because, to mankind, it is a matter of much more importance that the meanest wretch in the British dominions should live, than that a thousand "Cotton Lords" should wait for their cargoes even for twelve months instead of forty days. He has also promised to discuss "facts" at an early period. In the mean time, we would advise him to revise his "divisions," enlarge his "definitions," and to remember the dramatic precept of "qualis ab incepto," and not make himself ridiculous by his sempiternal contradictions. Let him not expect, in future, to succeed in the annihilation of contagion, by representing Nature bound hand and foot in the strings of logic, like Jupiter, as described by the Poets, tied up to the triangles of Fate or necessity. We can assure him she will prove an over-match for him, though he possess the strength of a hundred Reviewers, and will go on generating, propagating, and modifying contagion, despite of all human artifice, *in secula seculorum*. It is to be hoped, therefore, that instead of puzzling his readers with analogies between diseases and animals—acorns and brambles, and such stuff, he will condescend to speak intelligible language, and inform us, whether he thinks it more probable, that animals produced themselves or were produced by a superior power? whether it be more probable that bodies generate contagious diseases, or that contagious diseases produced themselves? whether, in short, it be more rational to suppose, that all contagions had, originally, an abstract existence, and, by some unlucky operation of chance, became subsequently allied to the human body? or that those contagions are the fatal offspring of the human body itself? Until he comes to a conclusion upon this subject, he must remain content with being the founder of a new sect, of which he will have the supreme happiness of being at once the priest, preacher, and congregation in his own person.

CANINE MADNESS.

To the Editor of THE LANCET.

SIR,

It is with great pain I find, by your publication of the 5th instant, that another case of hydrophobia has occurred and met with unsuccessful, or with what is worse, almost no treatment at one of the Borough Hospitals. This disease is called one of the 'opprobria of medicine,' but if any harsher term than opprobria could be applied, I think it would be appropriate, because a remedy has been discovered, the modus operandi of which can be easily accounted for, and its probable effects by that means calculated on. I allude to the use of the solution of lead, which seems to have been first exhibited with success by Dr. Fayerman, of Norwich, in the year 1819. He announced this fact to the public in a letter to the Editor of the Morning Herald, which letter was published in that newspaper of Saturday, the 7th of August last, and as I think the letter deserves as wide a circulation as can possibly be given to it, I have, in the hope that you will, for the sake of the advancement of medical knowledge, and of humanity, give it a place in your columns, sent you a transcript of it (for I have been unable to get a paper to send you). There can be no doubt that the Herald gave it pretty general circulation, and that it was read by numbers of medical men, but still, although many cases of hydrophobia must have since occurred, we do not hear that Dr. Fayerman's remedy has been properly tried. In your publication of 18th September last, there is a report of a case of hydrophobia at Guy's, in which, indeed, they exhibited, in nine hours, about forty grains of superacetate of lead, whereas in the same period Dr. Fayerman administered, I should think, about 200 grains, (at least 215 drops.) Had his decided example been followed, perhaps the patient's life would have been saved. In the other case, at Guy's, of the 25th of February last, it seems that they contented themselves with giving him a tobacco enema, and standing by and witnessing with conscious and hopeless imbecility the dreadful agonies of the

wretched patient, which might have been at least alleviated, had they known of or condescended to try Dr. Fayerman's remedy. Probably they are afraid that should they succeed in the cure, they might superinduce paralysis; at least this is an objection made by a medical man to whom I mentioned it. "I remember, Sir, (said he,) reading the letter you mention, but, if I mistake not, the treatment adopted was followed by paralysis, which is a dreadful thing, and leaves the patient, for the remainder of his life, a burden to himself and those around him; and therefore, rather than such a remedy should be adopted, I think it would be better to let the patient die, and there would be an end of his sufferings, at least as far as this life." But granting to this gentleman, and all other objectors on the same score, the force of his objection, still, in that case, the remedy would present a choice of evils, and the least of the two should be adopted; for what patient, labouring under the disease, if he were asked, as was very properly done by Dr. Fayerman, whether he would be cured at the expense of living paralysed, would not answer yes? There can be no objection to the remedy on the score of its danger, for the disease, if not checked, is sure to kill the patient, and that in a most horrible manner, while the proposed remedy can but kill him, and at all events, by mitigating the paroxysms, would smooth his passage to the grave. It seems a more rational and much more natural remedy than Magendie's, the merit of which is very problematical, and has never been attended with success in this country, and which is moreover difficult in its application, from the naturally restless state of the patient, and this difficulty was experienced in the case at Guy's in September last. Should you bring Dr. Fayerman's remedy into more extensive notice by inserting it in your pages, and another case should occur at the Borough Hospitals, we shall see whether the surgeons there will employ the remedy or let the patient die under the old methods, and I should hope you will keep your eye on them.

It would be no bad thing if your friend, Frederick Tyrrell, Esq. were to try the remedy on himself, for Dr. Fayerman says, that "violent affections of the mind, independently of corporal injury, in hysterical and hypochondriacal constitutions, have, at times, produced all the pathognomonic symptoms of canine madness;" and Mr. Tyrrell may feel himself assured, that the next time he experiences a fresh paroxysm of pseudo-hydrophobia, as above described, he will find it a *better* than the *Best* remedy he can find even in the Court of Common Pleas.

I am, Sir,
Your constant Reader and
Well wisher.

London, Mar. 15, 1825.

To the Editor of THE MORNING HERALD.

SIR,—The terrible effects resulting from the bite of a rabid animal cannot be sufficiently known or guarded against. If the following case is, in your estimation, of public importance, in consequence of the successful treatment adopted by me, and you will give it due publicity in the columns of your paper, you will confer an obligation on, Sir, your most obedient servant,

ARNALL THOMAS FAYERMAN, M.D.

Norwich, Aug. 1, 1824.

HYDROPHOBIA RABIES.

Charles Roberts, aged 42, and of stout habit, resided in the year 1819 in Hatfield-street, Goswell-street, London. In the evening of the 29th of May, returning home, he perceived two dogs fighting at the corner of the street where he lived, and he endeavoured humanely to separate them, in doing which, one of the animals, of the bull and terrier breed, turned round and bit him severely in the thick part of the right thumb. The wound bled profusely, and although he had not the slightest suspicion of consequences, yet he remarked the manner in which the dog quitted the combat with his antagonist, running furiously down the street. He did not think the injury he had sustained was of sufficient importance to require the aid of a surgeon, and the wound was merely co-

vered with a piece of adhesive plaster. For some days the bitten part was considerably inflamed, and attended with pain up the whole arm. These symptoms, however, subsided; after a few days the wound granulated, and the hand eventually became healed.

At breakfast, on the morning of the 12th of August, he was first seized with symptoms of hydrophobia. His wife, on pouring him out a basin of tea, perceived his countenance to undergo a remarkable change, he pushed his chair violently back, started up instantly, and clung to the mantlepiece of the chimney for breath, and displayed a degree of horror at the sight of the fluid on the table wholly indescribable. The wife, alarmed at this extraordinary change in his usual habits, called in the assistance of her neighbours. He still continued to cling to the mantle-piece, his feet rested on the fender; he implored with much earnestness the people around him to remove the breakfast things as speedily as possible; this being done, he requested to be led in the air; on approaching the door, he complained of a violent sickness, ejecting from the stomach a small quantity of bilious coloured matter. This effect instantly brought on a fresh spasm, accompanied with extreme sense of suffocation. At 12 at noon on the same day, I saw him, he was seated in an arm chair, displaying an appearance of a sullen dejection. I endeavoured to gain his confidence by language of a soothing nature, and, by zealous attention, I succeeded in my wishes. On learning the history of his case, I felt no hesitation in pronouncing it to be in my opinion a confirmed hydrophobic attack. I had some difficulty in making my patient believe it, and more so in enlightening him to a sense of his extreme peril. I examined the thumb and also the arm; around the part which had been bitten was a hard and firm cicatrix. There was apparently a slight blush of a reddish purple hue, and he complained of a tingling sensation, of a degree of numbness over the whole hand. He told me he felt an extraordinary sensation in rising from his bed in the morning, and going to the glass to shave himself, he felt so alarmed at the appearance of a horrid face looking over his shoulder, that he could not hold the razor to his chin. I laid before him the plan I intended to adopt for his recovery, but felt it an imperious duty at the same time to warn him of its probable failure. The bitten part was excised.

At two o'clock in the afternoon warm ablution was ordered for the space of fifteen minutes, and the potassa fusa (caustic pota-h) was applied to the wound, so as to produce a slough in the first instance, and afterwards a purulent discharge. I took from the opposite arm thirty ounces of blood, and gave him a scruple of china musk in a small quantity of currant jelly. Although the man remained tolerably quiet and easy for some hours after this treatment, and occasionally slumbered, yet there was an evident increase of nervous excitement. As evening approached, he complained of intolerable thirst and difficulty of swallowing his saliva. At six o'clock in the evening, I endeavoured with much earnestness to prevail on him to accept of a little weak wine and water through the spout of a teapot, so as to seclude from his sight the liquid I wanted to introduce. The naming the subject of fluid instantly produced a most violent convulsion; there appeared to be an evident sympathy between the fauces, the diaphragm, and the abdominal muscles. His face underwent during the spasm the most demoniacal contortions; still he was perfectly sensible, and complained of pain at the pit of the stomach. I advised his going to bed, which he instantly complied with, and, fully aware of his propensity to commit mischief either on himself or others, he suggested to me the propriety of his being under the subjection of a strait waistcoat; this was ordered, and after it had been put on he appeared more composed, and thanked God he was prevented the power of outrage.

I now took time to consider what was best to be done; my personal observations confirmed me in the previous idea which I had entertained, that hydrophobia is a disease specifically of the nervous system. I felt more strong in the belief from the knowledge that local irritation from wounds in irritable habits, especially when conjoin-

ed with a perturbed state of the passions, and also violent affections of the mind, independently of corporal injury, in hysterical and hypochondriacal constitutions, have, at times, produced all the pathognomonic symptoms of canine madness. Here was a case in point. Roberts, although of robust habit, was endued with remarkable sensibility, he was keen and quick in his perceptions, and of irritable temperament.

Having witnessed the powerful effects of lead on the nervous system, I determined at once to give this mineral a trial in the terrific disease before me. Previous to the exhibition of the anticipated remedy, I paid a fourth visit to my patient, at eight o'clock in the evening. I found him, at the moment, sensible; he told me he had slept, he supposed, half an hour, during which period he suffered from the most frightful visions; on awakening, he depicted, in glowing colours, the frightful state of the animal that bit him. His imagination represented him at the foot of the bed, eager to recommence the attack, armed, as he said, with four heads instead of one. This delirious feeling manifested itself during several minutes, in the most violent and disjointed expressions.

I took advantage of his subsequent calmness and exhaustion to state to him my desire to introduce a medicine unknown in the treatment of his malady, and I stipulated with him that he should have one arm at liberty, that he might take it himself; that force should not be used; neither should it be administered in any drink. I found no difficulty in getting him to comply with my proposal. At nine o'clock I gave him thirty-five drops of the liquor plumbi superacetatis, vulgo Goulard's extract of lead, on a lump of sugar. The pulse at this period was tremulous and irregular, and at 105. The power of deglutition, at this period, was greatly impeded by the frequent spasms affecting the glottis, and it was at least fifteen minutes before the medicated sugar had passed into the stomach. At ten o'clock the dose was increased, and he took forty drops of the extract of lead in the same manner as before; pulse 98. He slept from half past ten to within a few minutes of eleven. He was awoke by severe pain about the scorbiculus cordis, great thirst and heat about the fauces, but there was absence of spasmodic contraction, which had previously threatened suffocation.

At one o'clock on the 13th of August I repeated the venesection, eight ounces, and gave forty-five drops of the extract of lead mixed in a small portion of honey. At three this morning the dose was repeated, and, notwithstanding the powerful astringency of the medicine, there was certainly less difficulty in the act of swallowing. The pain about the stomach had been reduced in violence since the use of the lancet a second time, and the mind had become more calm and collected. At five o'clock, the thirst having increased beyond endurance, he expressed a desire to drink. A little weak brandy and water, mixed in a teapot, was presented to his notice, but the moment the fluid had been taken to the lips, a violent spasm came on. He seized the vessel with the fury of a maniac, and bit the spout off.

In twenty five minutes after this paroxysm had subsided, fifty drops of the solution of lead were administered. At nine o'clock complained of a coldness along the spine, and of a peculiar tingling sensation in the lower extremities, and soon afterwards of total inability to move his limbs; the pulse, at this time, was 84. I examined his legs, and found them completely paralysed.

The symptoms of hydrophobia became every hour after this crisis materially lessened. I fully succeeded at half past ten in getting down three table-spoonfuls of castor oil. I reduced the solution of lead to doses of twenty drops every three hours. At twelve o'clock the bowels were evacuated. At two p. m. we again attempted the introduction of the weak brandy and water. The patient made a bold and resolute effort to conquer or die in the struggle. He armed his mind with the strongest courage and fortitude. He carried the vessel to his lips, and although his countenance fully displayed the most horrid repugnance, yet, from the total absence of spasm, he succeeded in getting down

a considerable portion of the fluid. From this moment I considered the cause gained, and I hailed with joy the triumph which such a conquest inspired. I gradually descended the scale of my remedy to ten drops, and I had the satisfaction to find, that in the space of forty-eight hours from the first exhibition of the solution of lead in this case of hydrophobia, all the more urgent symptoms of this monstrous disease had abated. In four days not the least appearance of hydrophobic malady existed. The patient had the look of a person enervated and debilitated to an excessive degree. The wound in the hand was suffered to remain open for some weeks. On the 26th of September the patient recovered the use of his limbs, and was discharged.

(Signed)

ARNALL THOMAS FAVERMAN, M.D.

ROYAL COLLEGE OF SURGEONS.

The Lectures at this College commenced on Tuesday last; the Theatre was crowded in a very unusual manner, and the audience seemed particularly gratified on the occasion.—Mr. CHARLES BELL did the honours; and, in calling him to the Professor's Chair, the College people have exchanged the dull monotony of last year for something honourable to themselves, and attractive and profitable to the students; for, in Anatomy and Physiology, all who know Mr. BELL will award him the palm of excellence. The Lectures, however, are too demonstrative for publication, as every part of them requires its peculiar graphic illustration to render it intelligible or instructive to a reader.

THE STOMACH PUMP.

An impression very generally prevails, that the above instrument is of recent invention, and this erroneous belief has arisen from its not having been generally known until within the last eighteen months; in fact the public, previous to that period, were entirely unacquainted with it, and by far the greater portion of the medical profession in equal ignorance of its existence; and had it not been for the publicity which it received through the pages of THE LANCET, in all probability even to the present hour it would have remained a dead letter, as it had been for the *one hundred and twenty-five years* after its discovery! Several surgeons and instrument-makers have, within the last four or five years, laid claim to the merit of the invention; a statement of facts contained in a letter addressed to us by Mr. ALCOCK, an eminent surgeon, in Dec. 1823, clearly shows to whom that honour is due, which statement we will here re-insert for the information of those of our numerous friends who did not at that period peruse our pages.

Boerhaave proposed the use of the flexible tube passed into the stomach; that which Dupuytren and Renault perfected. Boerhaave published his proposal in the year 1698.

Renault's Dissertation, containing the description of the perfected instrument, in the year 1808.

1814.—Orfila's republication of Renault's description and experiments.

1818.—Orfila's work was translated into English and published.

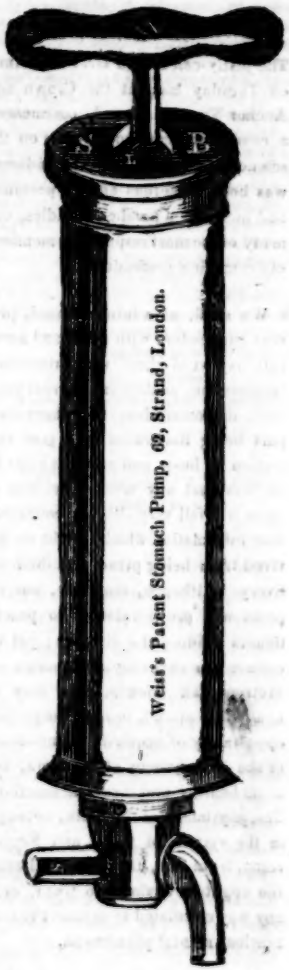
1822.—Mr. Jukes's observations and experiments published in the *Gazette of Health*; and, subsequently, in the *Medical Repository*, and *London Medical and Physical Journal*.

1822.—Mr. Bush's communication, published in the *Medical and Physical Journal.*

Thus it appears that the instrument was suggested one hundred and twenty-five years ago, that an account of it was subsequently published at various intervals, and that all the leading medical journals of *the day*, in the year 1822, published a description of it, and vainly attempted to do that which the influence of THE LANCET was enabled to accomplish by a single notice, viz. cause it to be universally known, and as universally adopted by the medical profession. To Boerhaave, then, is due the credit of having discovered THE STOMACH PUMP, and to THE LANCET the greater credit of having brought it into general use.

The pump, since its first adoption, has certainly been, by various alterations, very much improved, but by far the most material of these has been made by Mr. WEISS of the Strand, a correct sketch of whose instrument will be found in the adjoining column. Its chief advantages consist in there being no *valves* nor *stop-cocks*, and its acting equally well in any position in which it may be held; in fact it is so perfectly simple, that it might be used by a child. Where there are *stop-cocks*, they divide the attention of the operator, and *valves* are frequently impeded in their action by means of the intervention of portions of undigested food.

The pump is enclosed in a case, accompanied by flexible tubes, and the whole of the articles requisite for the removal of poison from the human stomach.



PHRENOLOGY.

The justly-celebrated Dr. SPURZHEIM on Tuesday last, at the Crown and Anchor Tavern, Strand, commenced a course of eighteen lectures on the science of Phrenology. The audience was both numerous and respectable, and included a number of ladies, and many of the most respectable members of the medical profession.

We shall, at a future period, present our readers with a full and accurate report of these very interesting lectures, but as they are in great measure demonstrative, the theoretical part being illustrated by a vast collection of busts and casts, it must be obvious that any written account of them will fall very short of conveying that information which might be derived from being present at their delivery. Although, therefore, our reports will prove valuable to practitioners residing at a distance; yet we cannot too earnestly recommend our Metropolitan friends, who may be enabled to attend, to seize the present opportunity of acquiring a knowledge of the *real* anatomy of the brain, and of its healthy and diseased functions. The physiology of the brain, as taught in the system of GALL and SPURZHEIM, is, in fact, the only one having the slightest analogy to truth, or in any way calculated to explain the multifarious mental phenomena.

HOSPITAL REPORTS.

GUY'S HOSPITAL.

Extraordinary case of Chronic Hydrocephalus.

H. G., the man who has been for so many months a pensioner at this Hospital and St. Thomas's, and the history of whose case we have given in Latin in the first number of Vol. II., expired on the 25th of last month. For some weeks previous to this he was slightly comatose, but within a few days of his death could give an answer to a question when roused; he became more and more insensible from the pressure of the immense accumulation of fluid in the head, and sunk as a person does in apoplexy.

An examination of the head took place, and Mr. KEY made a few observations on the case in a clinical lecture.

A cast of the head was previously taken by Mr. DE VILLE, at the request of the surgeons. What that industrious collector of skull-caps will be able to make of it, it is impossible to say, but here is a head large enough to satiate the most ravenous phrenologist—*thirty-three inches and a half* in circumference; sixteen inches from the tuberosity of the occiput to the root of the nose; and twenty inches and a half from the meatus aud. ext. of one side to the same point on the other side!

The head was just this size when he was five years old, but ossification was not complete over the anterior fontanelle when he first came into St. Thomas's, about fifteen months since, but it had become so afterwards. The parietal bones were thrown very much outwards, the right more than the left, and occiput carried almost horizontally backwards. The bones of the cranium were not thicker or thinner than ordinary, and the dura-mater appeared healthy; when it was laid open a large quantity of fluid gushed out, and the brain was observed lying at the bottom of this immense cavity, with its upper surface very much flattened. The corpus callosum was found defective, and a free communication was therefore established between the

ventricles of the brain and dura-mater. It is probable that this was a natural deformity, as the convolutions of the brain were not unravelled, as is usually the case when the lateral ventricles are much distended. The quantity of fluid contained within the cranium was rather more than *ten pints*, and of a pale straw colour. The inner surface of the dura-mater had lost its usual smooth glossy appearance, and was in some parts thicker and rather pulpy; no farther appearance of disease could be traced. Two drawings were taken of the parts, and the case altogether is more curious than useful; it shows what changes parts may undergo without destroying life, but no particular inference can be drawn from it.

Case of Fracture of the Spine.

W. D., a man aged 45, was brought into Accident Ward, on the 4th of the present month, having fallen from a scaffolding of about twenty feet, by which fall he had most unfortunately broken his back.

The poor fellow was quite sensible, and explained how the accident happened. His lower extremities were quite paralytic, and there was a numbness and aching pain in the upper; the respiration was also distressed, and principally carried on by the diaphragm. The urine is retained, and the fæces pass involuntary.

On an attentive examination, the upper portion of the dorsal vertebræ was found to be the seat of the injury, and most probably the first dorsal and also the seventh cervical are fractured. What was to be done? A *Comitia de Capite* was called, an operation was talked of, a discussion of some length followed, and it was agreed by the united talent of the "United Hospitals" that no operation should be performed!! Leeches were applied, to the number of six or eight, and a colocynth enema injected.

The patient is gradually getting worse, and it is not probable that he will live out the week. Mr. KEY, in his Clinical Lecture on Thursday, made some remarks on the nature of the accident, and produced some specimens of fractured vertebræ, in which the medulla spinalis had suffered considerable injury, and such it is *probable* is always the case. The inference deduced was, that the operation for the removal of the arch of the fractured vertebra is hopeless and useless, and that it was so considered by the majority of his colleagues. It is *possible*, however, that such extensive injury is not always done to the spinal marrow, as in the specimens then produced, and if only one out of ten can be saved by the operation, or even one in a hundred, it would be sufficient to justify the operation. The patient must die without it, and he may recover if it be performed. We confess ourselves rather surprised, that a surgeon of enterprize and ability, like Mr. KEY, should decline to hazard an operation in such a desperate case.

Operation.

The only operation performed here this week was the removal of an immense tumour from the neck of a woman, by Mr. KEY. The tumour had been forming some years, and was beginning to interfere with deglutition and inspiration. It was seated principally on the right side of the neck, and appeared to be composed of three principal lobes, one extending downwards by the side of the larynx and trachea, the second upwards, passing beyond the base of the jaw, and the third jutting outwards. It was very moveable, did not appear to be attached to any important structure, and was seated very superficially just between the integuments. Two semicircular incisions were made over the most prominent part of the swelling, and the integuments carefully dissected back; the finger was then introduced, by which it was pulled out and separated from some of its adhesions; a portion of the tumour projected under and toward the inner side of the base of the lower jaw caused some little difficulty to dissect it off, in doing which the facial artery was divided, but immediately secured.—No other vessel of consequence was wounded, and the flaps of skin were neatly brought together by sutures and straps of adhesive plaster.

The accidents admitted are numerous, but most of them unimportant.

ST. THOMAS'S HOSPITAL.

The principal subject occupying the attention of the persons here at present, is the daily squabbling between the Lecturers and the would-be Lecturers, as to who shall come in for the share of the "loaves and fishes." Mr. GREEN is Professor of Anatomy at the Theatre of St. Thomas's, and Mr. SOUTH, fired with the laudable ambition of sharing the Professor's Chair, stepped into the good graces of the Treasurer and divers of the Governors, as preparatory to taking his *seat*. Mr. SOUTH has succeeded in obtaining the appointment from the Treasurer, but he has found an opponent in Mr. BRANSBY COOPER, who thinks it very hard that he should not succeed to the honours of his worthy uncle the Baronet. To try the validity of the title of the parties "issue is joined." We shall again recur to the matter, and in the meantime we hope the jury will have returned their verdict.

Curious case of Tumour in the Axilla.

J. D., a man of sixty-five years of age, was admitted on the 10th of February, under the care of Mr. GREEN. His constitution, in the sense in which this term is generally used, is good; he is a robust muscular man, of a florid complexion; his habits have been, of late years, regular, and his occupation that of a butcher.

About six years since, he perceived a little tumour projecting from the right axilla, close to the outer margin of the pectoralis major muscle; its formation was unattended with pain, or any evident pulsation, and perfectly globular. This has gradually increased in size to the present period, but still retains its spherical figure, and is now about the size of an ordinary cricket ball. When the arm is lifted up, the tumour appears almost entirely in the axilla, but when allowed to hang by the side, the tumour appears to be principally seated beneath the pectoralis major. So much then for the general appearance and history of the said tumour; and now we come to a question which has puzzled the three surgeons of St. Thomas's to answer. Is this an enlarged absorbent gland, an encysted tumour, or aneurism? We have said enough of its history, and shall proceed to describe the other circumstances which may assist our diagnostic. The tumour has not the firm resisting feel afforded by an old aneurismal sac; the circulation in the arm is not at all impeded; the pulse is as firm and regular at the right wrist as the left; there is no numbness, at least no permanent numbness, in the extremity, nor any impediment presented to its ordinary motions; it is without pulsation to the touch, and what may be considered more confirmatory of its being unconnected with a large blood vessel is this, the application of the *stethoscope* does not impart that peculiar sound which the passage of a current of blood into an aneurismal sac invariably occasions, and which has been called by Laennec the *sifflement* from its close resemblance to the sound communicated by the instrument on the passage of the air into the lungs. Pressure on the subclavian artery does not diminish its bulk, and it does not appear to be connected with the surrounding structure by an extensive base.

These then are our reasons for supposing that it is not aneurism, and we shall be content to observe the practice which the body surgical may determine on. Mr. GREEN, who never says anything very decidedly, *suspects* that it is aneurism, and Mr. TRAVERS is *inclined* to coincide with the *suspicions* of his colleague, whilst our friend, Mr. TYRRELL, is firm in asserting that it is not aneurism, and we hope his opinion will prove correct.

Operations.

The only operation performed for the week was the following, by Mr. GREEN:—The patient has been afflicted with stricture for many years, and has been in the house since October last, during which time various attempts have been made to remove the stricture; common bougies, metallic and caustic bougies, have been in their

turn resorted to without affording relief, and as the difficulty experienced in emptying the bladder had at last been exceedingly great, amounting almost to retention, he was advised to submit to an operation.

The patient was seemed in the manner usually adopted for the performance of lithotomy, and a grooved staff having been introduced into the urethra as far as the stricture, Mr. GREEN made an incision on the left side of the raphe, extending from just below the scrotum two inches downwards, and then having felt the extremity of the staff with his finger, carried the point of the knife into the urethra and cut upwards towards it; he then tried to carry the staff onward, but was unable to do so; it was then withdrawn, and a small sized catheter introduced, but it could not be pushed farther than the other instrument. The fact was, that the stricture extended farther in the urethra than the point at which the knife first entered, and therefore Mr. G. was obliged to dilate it toward the bulb, after which the catheter was easily passed into the bladder.

The patient has taken some castor oil two or three times since the operation; a catheter is worn in the urethra, and the case is going on very favourably. There is no doubt but that this operation will, in a short time, supersede that of puncturing the bladder, in cases of retention of urine produced by stricture, unless that stricture be seated very high up indeed, and even then it is probable that the same operation as that by which a stone is now extracted from the bladder would be preferable to the other disagreeable and merely palliative operation.

Several accidents have been admitted, and some of a very serious nature. We shall give a report of Mr. TYRRELL's strangulated femoral hernia case next week.

MIDDLESEX HOSPITAL.

March 16. On Friday last Mr. JOE BURNS removed a malignant tumour from a man's lip. We wanted the curiosity to be present at the operation, and consequently shall say nothing about it from hearsay.—Caveto Joberns.*

[ERRATUM.—In our last report, page 317, 2d column, for "wants some of the characters of a hernia, viz. swelling," read "wants some of the characters of a *hernial swelling*.]

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* Caveto Joberns—"Now take your time, Joberns!" Mr. Cartwright's advice in a quondam hare-lip case.—Unpublished MS.

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The injury done to the extremities, by the former mode of elongation, is avoided, the improvement of the form is necessarily more rapid. There are not any cases, however calamitous, which cannot be considerably relieved. There are very few in which the Patient cannot be completely cured.

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THE LANCET.

The FIRST VOLUME of this Work is now reprinted, and is ready for delivery.

The Second Volume is in the Press.

Price Sixpence.

CUT UP OF THE REAL SIMON PURE, BY H. BROUGHAM, Esq. IN A CAUSE

TYRRELL *v.* THE EDITOR OF THE LANCET.

On Saturday last was published, printed uniformly with THE LANCET, containing 48 Pages, and forming a Supplement to the last Number,

A VERBATIM REPORT of the above TRIAL, which took place in the Court of Common Pleas, Guildhall, London, on Friday, February 25th, 1825. The admirable Speech of Mr. Brougham contains a most able and accurate Dissection of one of the "THREE NINNYHAMMERS" of St. Thomas's Hospital.

Published at THE LANCET Office, 210, Strand; and may be obtained of every Bookseller in the Kingdom during the next six months.

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